

Recent Publications

Financial Stability, Economic Growth, and the Role of Law. By Douglas W. Arner. Cambridge: Cambridge University Press, 2007. Pp. 344. Price: \$37.99 (Paperback). Reviewed by Peter Liem.

Adam Smith assumed that, even when it meant less profit, “every individual endeavours to employ his capital as near home as he can,” where “[h]e can know better the character and situation of the persons whom he trusts, and if he should happen to be deceived, he knows better the laws of the country from which he must seek redress.”¹

Whether or not Smith’s assumptions were correct at the time, now investors will go wherever profit is to be had. Yet the dangers of overseas investment, to which Smith pointed, still remain today. Douglas Arner’s new book addresses the current international and global mechanisms designed to mitigate the dangers about which Smith warned. Starting with the financial crises of the 1990s in Latin America and Asia, Arner discusses the goals the financial world set for itself and the adjustments it made to the prior international system of finance in order to increase and stabilize transnational investment flows.

Unfortunately for the reader, Arner’s book awkwardly straddles a line between a textbook and a meaningful policy recommendation, with the latter winning over the former because of Arner’s diffuse presentation of his main themes. Although he sets out to argue that law (including associated institutional frameworks) is a key foundation for economic development and that an “effective” financial sector is important for economic growth, Arner makes neither argument persuasively. Substantial parts of the book are simply dedicated to reporting the ongoing work of the world’s financial policy agenda-setters, sometimes in relatively intricate detail. While this may be useful for anyone interested in the precise workings of, say, the International Monetary Fund (IMF), it takes away from the substance of Arner’s main claims. Arner provides what could be a useful text for a course on international financial development, but a poor defense for the position that the financial industry as we know it is necessary or even good for economic development.

Arner describes the various international finance regulators striving for two things: transparency and consistency. The detailed description of the role of law in building, maintaining, and strengthening investor confidence through transparency and consistency presents a strong factual case for the importance of rule of law in preventing financial crises and for promoting economic development. Arner is much less persuasive, however, when he tries to make the normative case for the importance of rule of law in the context of global and transnational finance. For example, Arner speculates that the need for financial regulatory law and strong institutions was “not

1. ADAM SMITH, *AN INQUIRY INTO THE NATURE AND CAUSES OF THE WEALTH OF NATIONS* 475-76 (Edwin Cannan ed., University of Chicago Press 1976) (1776).

given significant attention in emerging, transition, and developing economies until the mid 1990s, largely because they existed already in the developed economies and hence were often ignored or assumed in the context of financial research looking to the developed country context" (p. 92). However, the relevant consideration should be what the now-developed countries did while they were in developing stages, as the legal and institutional frameworks present then were often significantly different than they are today.

Arner also discusses some of the less-discussed legal structures needed to support a stable financial sector, such as strong intellectual property rights. He aims to provide a strong argument for the need for a sophisticated and adequately planned legal infrastructure for development to take place. However, here the discussion is lacking in depth. For example, it is questionable whether developing countries should have strong intellectual property protections, given that many developed countries flouted patent laws to develop domestic industries using the very latest in foreign technology.² Many of the discussions throughout the book suffer from a similar failure to address rather obvious counterexamples to the notion that a particular legal framework is going to promote development.

Arner is modest in his recommendations for how best to strengthen financial stability globally. He limits himself to comments on studies and reports from agencies such as the World Bank and the IMF, and either endorses them wholesale or just slightly modifies their conclusions. His broadest recommendations come in the form of his approval of developing global systems for regulating finance. He suggests that the Bretton Woods system needs to be readdressed in an effort to mitigate the dangers of national financial crises spreading to the international level (p. 336). His careful and detailed discussions leading up to this idea make for his best argument, although the mass of detail without structure is an impediment that prevents Arner from being fully persuasive.

Undoubtedly, financial stability is to be preferred over the threat of financial crisis, but the easiest path to stability is to shut financial markets down altogether. Arner consistently points out that "integration into the financial system is not without dangers and must be based on coherent sequencing of liberalization preceded by the development of an effectively functioning financial system in each country as a necessary first stage" (p. 303). A question naturally arises: why should we aim to introduce or expand financial markets in developing countries? The reader may be frustrated by the lack of an answer to the question of whether or not high finance is actually good for development. Arner devotes two chapters of his book to this question, but he never answers it adequately. It is not a given that the development of a sophisticated financial industry improves life in developing countries. Arner does mention China as a positive example, but China has been extremely careful in its regulation of finance, and it continues to strictly regulate foreign investment in Chinese banks to this day. The facts of China

2. See, e.g., Ha-Joon Chang, *Intellectual Property Rights and Economic Development: Historical Lessons and Emerging Issues*, 2 J. HUM. DEV. 287 (2001).

do not support the notion that anything other than a tightly controlled financial sector is good for economic growth.

Part of the problem may be that Arner fails to distinguish adequately between the interests of investors and those of the general population. The groups have little significant overlap, especially when it comes to international investors and average citizens. Arner flags this problem immediately when he notes that “economic development” is a contested term (p. 13). This is surely a familiar point to anyone acquainted with the controversy as to who benefits from the policies of the Washington Consensus, but Arner lets the matter drop with only sparse mention in the rest of the book. For instance, in his first discussion of company law, Arner off-handedly equates advancing corporate development with advancing economic development, but does not explain why these two are one and the same (p. 116).

In any discussion of economic development, the question of who benefits must always be at the fore. A greater sensitivity on Arner’s part as to how particular legal and institutional structures benefit or harm different groups of people would have made for a much more useful volume.

The Most Noble Adventure: The Marshall Plan and the Time When America Helped Save Europe. By Greg Behrman. New York: Free Press, 2007. Pp. 448. Price: \$27.00 (Hardcover). Reviewed by Marisa B. Van Saanen.

The Most Noble Adventure: The Marshall Plan and the Time When America Helped Save Europe is a rich narrative detailing the creation of the Marshall Plan in the wake of the Second World War. Author Greg Behrman, who describes his book as the first narrative history of the Marshall Plan, depicts in unsparing detail the personalities, actors, and historical forces that produced what is arguably the greatest foreign aid success story in history. The book is largely sympathetic and celebratory in tone, uncritical of its characters and descriptive perhaps to a fault. But it is an intimate and detailed portrait of the political figures that created the Marshall Plan.

The story is timely, released amid war and reconstruction in Iraq, and deep debates about the future of development aid and the role of the United States in international relations. It also comes at a time when the Marshall Plan is often referred to nostalgically as an example of when the United States got foreign policy right. The book seems meant to inspire and invite another Marshall Plan-like venture, with Americans taking the lead in enacting a foreign policy that reconstructs, renews, and helps unite the world.

“Perhaps it is worth remembering,” Behrman writes, “a time when extraordinary men summoned their vast talents, wisdom and creative potential to design and execute a plan with a transcendent aim; when America’s foreign policy was defined, at once, by its strategic interests and the very best of its ideals, when Americans were infused with a shared sense of mission and purpose—so much so that it roused the national spirit and elevated America’s own conception about what it could accomplish, what it could be” (pp. 341-42).

The portrait Behrman draws of the creation of the Marshall Plan is one of admirable and visionary statesmen, disciplined, yet compassionate, strong as well as perceptive and responsive to the dire situation facing the devastated post-World War II Europe. Behrman tells the story of the four-year, thirteen billion dollar program (more than one hundred billion in today's dollars) through the personal and professional stories and roles of George C. Marshall, Will Clayton, Arthur Vandenburg, Paul Hoffman, Richard Bissell, and W. Averill Harriman. Marshall, most notably, is painted not only as a figurehead of the Marshall Plan, as in some historical accounts, but also as an engaged and insightful decisionmaker at the center of plans to renew and rebuild Europe.

The prologue takes readers to Cambridge, Massachusetts on June 5, 1947, to Harvard University's 286th Commencement and what Behrman describes as, "the first 'fully normal'" graduation ceremony since the United States entered the Second World War, five and one-half years earlier (p. 1). On that day, the newly appointed Secretary of State, General George C. Marshall announced that America was ready to stand with Europe in its rebuilding. To an unsuspecting audience, he delivered a speech setting out the preliminary framework for the plan that would eventually bear his name. Marshall's speech was an invitation to Europe to request support from the United States—"the initiative, I think, must come from Europe," he said. (p. 69). The idea was that the United States would pledge financial support and partnership with Europe, but leave Europe in "control of its own destiny" (p. 69). At the end of his speech, Marshall spoke freely, looking finally to his audience: "We are remote from the scene of these troubles. . . . Yet the whole world's future hangs on a proper judgment . . . of just what . . . can best be done, what must be done" (p. 70).

From this starting point, the book goes on to describe, in vivid detail, the negotiations in Washington and Europe that led to the Marshall Plan. Most of the book is devoted to detailing these negotiations and describing the larger political and economic forces reverberating in a world devastated by war. The author describes the Marshall Plan both as a force meant to stave off communism's further spread in Europe and as a political and economic gesture that helped inspire European integration and economic cooperation; later, it was seen to have inspired the United Nations Development Program (UNDP) and other international development efforts. "Offered in humility as a hand in partnership, it sought nothing less than to refashion Europe in fundamental and audacious ways" (p. 5). The book focuses both on this idealism and the strategic goals and interests it served, including financing some of the first covert intelligence operations in Europe and helping, perhaps, to create a world ripe for the Cold War.

The book's only substantial weakness is that it tells only the American side of the Marshall Plan. Behrman is quite explicit that he aimed to write the American perspective, but this one-sidedness makes for an incomplete account. The way the Americans diplomats are described seems overly congratulatory and the subtitle, "The Marshall Plan and the Time When

America Helped Save Europe,” might be read as a bit patronizing by non-Americans.

Another weakness is that the stories of the American diplomats are engaging but sometimes too detailed and drawn out. We learn of Marshall’s hazing at the Virginia Military Institute where his classmates learned of his courage by forcing him to squat, until he eventually fainted, over an unsheathed bayonet. We learn that Clayton abstained from alcohol until the age of 58, when his son, a doctor, encouraged him to drink sherry for his health, and that Harriman, with his “slicked jet-black hair and black eyes,” attracted a “bevy of women admirers” (p.191). All of this seems a bit unnecessary and detracts from the seriousness of the tone.

All that said, the book does shed considerable light on the people involved in the creation of the Marshall Plan and provides a comprehensive account of the ways that individuals helped shape foreign policy that changed the world. In a world at war again today, this book holds lessons for all of us.

Islands of Agreement: Managing Enduring Armed Rivalries. By Gabriella Blum. Cambridge: Harvard University Press, 2007. Pp. 277. Price: \$49.95 (Hardcover). Reviewed by Karen Grohman.

Gabriella Blum’s *Islands of Agreement: Managing Enduring Armed Conflict* develops and explores the intriguing premise that the traditional dichotomy between war and peace should be abandoned in favor of recognizing that stable areas of interaction exist between many states engaged in protracted armed rivalries. According to Blum, these “islands of agreement,” which include such disparate phenomena as transnational bus lines and temporary ceasefire agreements, have the potential to foster peace by allowing belligerents to reach agreements in areas unrelated to the source of the rivalry, without compromising their positions on their central disputes.

Blum puts this premise to the test in examining “islands of agreement” in three “enduring armed rivalries” that have defied all attempts at resolution. These are the long-running conflicts between India and Pakistan, Greece and Turkey, and Israel and Lebanon. Blum argues that arbitrators should seek to “manage” these conflicts, rather than solely looking to “resolve” them (p. 17) by adopting strategies and decisions that aim to limit their scope and effects.

Unfortunately, Blum never makes clear what exactly the goals of conflict management should be. Should countries focus on reducing civilian casualties, encouraging cultural exchanges, sustaining economic growth, or perhaps developing regional solidarity? Her studies of the three conflicts illustrate all of the above, but the work would be improved by a clearer set of specific initiatives for which conflict managers—either domestic or international—should aim. The other tension in her work, which she acknowledges and addresses at length, is that her approach may actually *prevent* a permanent settlement of fundamental disputes by resulting in a “universal affirmation” of them (p. 46). This tension in fact threatens her entire thesis: if these disputes are accepted as the status quo, what is to

encourage countries presently at war to settle their conflicts through the accepted mechanisms of compromise, arbitration, or third-party resolution? Blum's analysis correctly notes that, in her case studies, such mechanisms have thus far been unsuccessful in achieving their aim of peace. Is this, however, reason enough to abandon them entirely and let the fundamental *casus belli* rest unchallenged? Blum's work cannot affirmatively answer this question.

Blum begins by analyzing India and Pakistan, whose rivalry dates from the partition of British India in 1947. Although the fundamental conflict regarding Kashmir remains intractable, Blum identifies several longstanding islands of agreement such as water usage rights, the implementation of wide-ranging intercultural initiatives, and resolutions on issues ranging from airspace regulations to cross-border bus travel. Blum admits these islands of agreement are not pervasive and notes a few examples of discord in areas separate from the border conflict. For example, Pakistan remains opposed to allowing free trade with India.

Blum's thesis is weakened, however, because she offers no evidence that these islands have allowed an overall normalization of relations between the two countries. Nor does she address the fundamental problem of potential for nuclear war. No matter how many bus lines facilitate the transfer of people from one section of Kashmir to the other, the possibility remains for the use of weapons of mass destruction in the face of a perceived threat to the national security of each country. Blum's proposed islands speak to a less urgent threat by prioritizing cultural normalization and do not address the fundamental issue that is of the most concern.

Blum next examines Greece and Turkey, whose disagreements arise over maritime boundaries in the Aegean Sea and the status of the divided island of Cyprus. She argues that islands of agreement established outside these areas of conflict have been significant enough to produce what amounts to normal relations despite the ongoing core disputes. This case differs from the others in that these islands of agreement have expanded to encompass almost all of the interactions between the two states, leaving Greece and Turkey in a state no longer resembling war. Though each still participates in military exercises designed as a warning to the other, their economic and diplomatic relationship is stable. Greece has withdrawn all of its objections to Turkey's membership in the European Union and Cyprus is already a member state.

Blum notes a few areas that are still problematic—such as school curricula in each country that continues to cast the other in the role of the “enemy”—but on balance, the two states have come to enjoy stable relations. This seems to be the best result that her version of conflict management can accomplish.

In her third study, Blum turns to far thornier terrain: the conflict between Israel and Lebanon. Unlike the other two studies, she finds only one island of agreement: the 1996 Israel-Lebanon Ceasefire Understanding. The Understanding established rules of engagement designed to protect civilians in southern Lebanon during the Israeli occupation. Blum argues that this

agreement succeeded far longer than expected because it limited itself to protecting civilians rather than aiming to end the conflict, and that where it was too far-reaching, it may have actually exacerbated the conflict.

The Israeli-Lebanese Understanding is useful both in its application to the specific conflict and its implications for Blum's overall theory. Because it went further than was practical in attempting to protect civilians—for example, barring fighting within a certain distance of residences practically amounted to outlawing the guerilla war (p. 248)—the Understanding eventually collapsed. The lesson is that plans that aim to end the conflict and address fundamental disputes accomplish less than narrow islands of agreement that do not overreach. This is strong evidence for the workability of her theory as applied to real conflicts.

One difficulty with the work is its dismissive treatment of the involvement of the international community and its potential role in implementing the approach she favors. Blum's analyses focus primarily on domestic arrangements and solutions, such as bilateral agreements and internal modes of enforcement among the parties concerned. Blum's position is based on traditional state concerns over sovereignty that make them reluctant to submit to third-party tribunals, as well as the occasional inapplicability of international rules to specific contexts. Yet international standards and review could provide another framework for managing these conflicts. For instance, international legal standards and the involvement of the international community helped shape islands of agreement between India and Pakistan. Similarly, France and the United States played a major role in sustaining the Israeli-Lebanon Understanding. Thus, rather than aiming her argument solely at scholars and domestic politicians, Blum would also do well to appeal to international actors capable of shifting to a global focus on "management" where appropriate.

Overall, Blum's analysis is informed, scholarly, and persuasive. Her study ultimately meets its goal of articulating "the rich, often untapped potential of interstate relationships" (p. 267). Her work can guide politicians seeking to manage conflicts, rather than laboring in vain trying to end them. In the end, although islands of agreement will not yield a "dramatic change over the core conflict" (p. 268) or create a complete peace in the traditional sense, Blum demonstrates how significant opportunities lie at the margins to reduce the scale—if not the scope—of core conflicts.

The J Curve: A New Way to Understand Why Nations Rise and Fall. By Ian Bremmer. New York: Simon & Schuster, 2007. Pp. 323. Price: \$15.00 (Paperback). Reviewed by Scott R. Anderson.

The word "parsimony" generally has a negative connotation. But since Kenneth Waltz first reduced international relations to a simple collision of billiard balls, some writers on global politics have come to see it as more of a virtue. They argue that paring theories down to their essential variables and causal links makes them more readily comprehensible. As a result,

parsimonious theories find broader audiences and better reach the ears of public figures too busy for the detail-heavy work of conventional academics.

Few authors have employed this strategy more effectively than Ian Bremmer in his most recent work, *The J Curve: A New Way to Understand Why Nations Rise and Fall*. From a simple visual model, Bremmer extracts surprisingly complex prescriptions for a number of pressing international problems. But the same simplicity that has made his book a popular sensation also lends it some significant shortcomings.

The book centers on a two-dimensional graph with political stability on the vertical axis and openness—representing the free flow of goods and ideas—on the horizontal. Correlating the two results in the book's namesake curve, declining steeply from the left to its nadir before beginning a gradual climb upward and to the right. For each state, this J curve represents the potential range of its political development: from a stable but closed autocracy to a developed, open state. Every country has a unique J curve depending on its specific historical, cultural, and economic circumstances, and these curves shift vertically in response to external shocks and windfalls that affect how easily stability can be achieved at every given level of openness.

Bremmer's key observation is that, for a state to transition from one side of this curve to the other, it must traverse a span of major political instability. For closed states, the costs related to this period severely tax leaders' political capital and encourage them to seek more immediate security by retreating up the steeper left side of the curve. Economic shocks aggravate this trend by increasing the costs of opening, while windfalls can help alleviate it by providing leaders with additional resources.

This logic informs Bremmer's main policy critique regarding the counterproductiveness of targeting autocracies with economic sanctions, political isolation, and threats of regime change. He notes that these policies only create a destabilizing downward shift in the targeted country's J curve, justifying the closed conditions in which autocratic regimes thrive. Instead of isolation, Bremmer argues for engagement: pressure to open from the international community paired with political and economic support capable of raising targeted countries' J curves and staving off destabilization.

But Bremmer does not limit himself simply to broad, international concerns. Through twelve individual case studies he also deconstructs the challenges facing specific nations and provides recommendations on how they can best achieve the long-term stability openness promises. Noting the Castro regime's carefully cultivated economic hegemony over Cuba, he recommends an end to the U.S. embargo. Because of Russia's successful escape from economic disaster under Putin's heavy hand, he encourages a leap for fuller democracy. Iran, North Korea, Venezuela, and countless other problem states receive similar treatment and prescriptions that match the unique challenges they face.

These case studies are both the strength of Bremmer's work and the clearest indicator of its weakness. Each ably synthesizes and addresses the conditions facing each country and provides pragmatic and reasonable policy prescriptions. But aside from scattered, largely superficial references to curves

shifting this way or that, none are particularly tied to the theory that is supposedly at the core of the book.

The problem lies in Bremmer's overly broad independent variable, openness. Through it, he represents freedom of information, economic liberalization, civil rights, education reform, democratization, and other quite distinct phenomena. While his model would imply that these are all effectively interchangeable in their relationship with stability, Bremmer himself admits this is not the case, noting, for instance, that "political openness is a better guarantor of long-term stability than economic openness" (p. 259).

Throughout his case studies, Bremmer finds himself having to differentiate between these different forms of openness. When dealing with Saudi Arabia, for example, he promotes a policy of "education first—then democratization," fearing that sudden political openness would only lead to a more closed, less stable state, given the Saudi public's limited experience with popular governance (pp. 122-25). But such nuanced distinctions aren't made in the J curve model. To put forward the policies he sees as most fit, he must depart from it, or, at the very least, ignore it.

This same problem handicaps Bremmer in actually explaining why the J curve takes the shape it does. While the source of stability for autocratic police states is not hard to discern, he only hints at why openness leads to greater stability in the long run, noting that "[t]he stability of states on the right side of the curve depends on institutions," (p. 6) and that "[c]onsolidated democratic regimes . . . are the most stable of states" (p. 11). A generous reading might take this to mean that the unpredictability of open states combines with market pressures to promote effective institutions as a means of risk management, but Bremmer never goes so far. Developing such institutions, as well as those other factors that promote stability, depends on a complex network of factors including economic and political liberalization, rule of law, and material resources, that would inevitably cloud his concise model. Instead of fully incorporating these factors into his theory, Bremmer chooses to merely aggregate them into irrelevance.

This choice limits the efficacy of the J curve theory. Absent a more nuanced theoretical treatment of the factors actually necessary for stability (something on par with the policy analyses of his case studies) it can do little to predict or explain specific political outcomes. At best, it seems suited to a descriptive role, providing a way of viewing and interpreting political trends without explaining why given events occur as they do. While not without value, this is a far cry from the "new way to understand why nations rise and fall" promised by the book's subtitle.

But perhaps it's enough to satisfy Bremmer's purpose. An accomplished political scientist, Bremmer is no doubt familiar with the requirements of a true causal theory. Indeed, he himself avoids labeling the J curve as such, instead describing it as an "ordering principle" and "a framework with which to understand the pressures and motivations that guide . . . countries' leaders" (p. 24). Painting his picture with broad strokes may require Bremmer to sacrifice causal logic, but it does leave ample room in his simple framework

for discussing what would otherwise be disparate policy proposals. And perhaps this is precisely what Bremmer was aiming for: a sugar coat of simplicity for his diverse bundle of ideas, one that would make them more palatable for their intended audience, general public and elite alike.

By this measure, the proliferation of Bremmer's work alone marks it as a success. The only danger is that readers will fail to see the limited scope of the J curve theory for what it is. Using just the J curve, one can't necessarily see why pushing countries toward open markets, social liberalism, and democracy can cause problems as long as enough money is expended to keep the country afloat during instability. Nor can one tell whether a period of instability is merely "free people . . . free to make mistakes" on their way to an open, stable future, as Defense Secretary Rumsfeld once described looters in Iraq, or signs of a more enduring crisis. Bremmer himself warns against such radical interpretations, noting at one point that "[t]he damaging effects of pushing for comprehensive change in a society that isn't ready for it can last for years" (p. 269). But determining when a society is ready involves factors and variables that have no place in his theoretical schema, leaving a great deal of room for dangerous misapplication.

Perhaps the best lesson that can be taken from *The J Curve* is tied to this last point and the general difficulty Bremmer faces in building a true causal theory: that the history of human civilization is a complex, chaotic, and bloody affair resistant to broad generalizations. Wherever men try to force history to align with their theories and ideologies, it snaps back with a wave of unforeseen consequences. Thus, the best way to engage the future is through slow progressive steps, guiding history by building on the developments and institutions of the past instead of tearing them down to construct anew.

Though this gradualist view isn't reflected in the J curve itself, Bremmer embraces it in his commentary and the myriad of pragmatic policies he advocates for in his case studies. One can only hope that readers don't lose track of this perspective through the parsimonious sheen Bremmer applies to it and walk away with yet another flawed theory of political development that promises more than it can deliver.

Rulers, Guns and Money: The Global Arms Trade in the Age of Imperialism.

By Jonathan A. Grant. Cambridge: Harvard University Press, 2007. Pp. 288. Price: \$49.95 (Hardcopy). Reviewed by Nwamaka Ejebe.

In *Rulers, Guns and Money: The Global Arms Trade in the Age of Imperialism*, historian Jonathan Grant takes an atypical approach to the arms trade in the years leading up to the First World War. Eschewing the traditional focus on the Great Powers, Grant goes beyond Europe to provide a rich and insightful account of the global arms trade and its consequences from 1860 to 1914. Focusing on a time of rapidly advancing weapons technology and shifting imperial agendas, Grant meticulously researches what motivated states to export and import arms.

Starting with the basic premise that private arms dealers needed foreign consumers to remain profitable, and nations across the globe sought to modernize their militaries, Grant interrogates the complicated relationship between Western European and North American arms exporters, and buyers in Eastern Europe, Asia, South America, and East Africa. Contrary to the conventional wisdom, which holds that arms manufacturers are the servants of their states, Grant arrives at the unconventional conclusion that “industrial and banking interests often found themselves at odds with diplomatic or military-strategic interest, with the results showing that generally the history of arms trade did not conform neatly to the diplomatic history of the period” (p. 3).

Drawing on intelligence reports, diplomatic correspondence, and business documents in addition to region-specific secondary sources, Grant deals with a wide range of issues in only a few hundred pages. The work as a whole is divided thematically and geographically with each chapter highlighting an important aspect of the global arms trade narrative. Grant focuses first on the weapon modernization efforts in four autocracies—Russia, the Ottoman Empire, China, and Japan. He explores the motivation of each nation for armament, as well as their approaches to financing imports, developing domestic manufacturing capacities, and maintaining relationships with suppliers. Second, the work explores the nature of the secondhand arms trade in the Balkans, Ethiopia, and Djibouti, its role in nation-building, and its relationship to imperialist aspirations. Third, Grant shifts into a discussion of the nature of diplomacy, political systems, and German and Austrian private arms sellers in Eastern Europe. Fourth, he offers a comparison of regional naval races during the dreadnought era in Asia and South America. Finally, Grant examines the fortunes and the misadventures of the German arms manufacturer, Krupp, and its main competitors.

With such an expansive coverage of nations and events, Grant’s narrative sometimes seems disjointed as he quickly jumps back and forth across space and time. Nevertheless, the loss of continuity is an acceptable price for the insights gained by Grant’s global survey and his comparative historical approach. Grant is most poignant when he draws on the patterns witnessed in individual nations and offers generalized conclusions on the nature of the arms business. For example, the counterintuitive politics associated with arms procurement is a recurring theme in Grant’s work. In his analysis of the dreadnought races in South America and the Eastern Mediterranean, Grant concludes the races “cast serious doubt on the notion that democratization reduces armed competition and automatically promotes peace,” noting that “[t]he introduction of even a limited democracy into the procurement process made possible the employment of naval acquisitions as a sign of popular will and political leadership, thereby transforming the arms trade into an arms race” (p.188).

Grant also explores the nexus between international arms trading and imperialism. At the end of the nineteenth century, he notes, imperialism served both as a cause and an effect of the arms trade. The efforts by European powers to secure colonies in Africa and spheres of influence in Asia forced some nations to arm themselves against foreign encroachment, while

the influx of arms convinced others to participate in their own imperialist projects. Nevertheless, Grant refutes the notion that European powers were able to effectively leverage arms trading to influence buyer nations. Grant shows how Ethiopia's emperor, John Menilek, manipulated the Italian government into selling rifles to Ethiopia. While Italy assumed that the arms would bring Ethiopia under Italy's sphere of influence, in fact just the opposite occurred. In 1896, when Italy attempted to invade the country, Menilek used his army's Italian firepower to defeat the advances.

Grant's historical account also contributes to the contemporary discussion on arms trading. Today's social scientists use two models to describe arms races: the action-reaction model and the domestic structure model. The action-reaction model focuses on the competitive interplay between states, while the domestic structure model emphasizes the internal political and economic dynamics of the state. While Grant concedes that both models are persuasive, he finds them flawed because their "state-centric focus omits the business dimension from the analysis" (p. 242). As Grant's work deftly illustrates, with examples from each geographical region covered, the business dimension is essential to understanding why buyer states chose to purchase armaments from particular supplier nations. He explains that while diplomats played a role in promoting arms trading, it was the private firms who aggressively recruited and secured foreign customers. With, and sometimes even without, their nation's official support, private arms sellers developed close relationships with foreign military officers and political leaders to broker arms deals. Moreover, while ambassadors typically encouraged arms trades with allied-nations, private firms driven by profit considerations advocated for arms deals with friends, potential allies, and rival nations.

As the world's top arms trader to the developing world today, the United States has a particular interest in understanding the role arms trading plays in domestic and international fronts. Despite the significant influence it has on the economies and politics of both exporter and importer nations, the current arms trade, for the most part, remains shrouded in mystery. Historical works like Grant's, therefore, are useful guides into this hidden world. Similarities between Grant's history and the present can easily be drawn. For example, like their predecessors, today's top manufacturing nations (the United States, Russia, Britain, Germany, and China) continue to use arms trading to exert political influence and continue to achieve mixed results. However, these parallels are limited. In nineteenth-century Ethiopia, arms maintained the sovereignty and viability of the state; however, in twenty-first-century Africa the widespread availability of weapons serves as a destabilizing force and the AK-47 as a symbol of revolution and insurrection. With the exception of his business dimension prescription, Grant provides no additional insights into how his work can inform our understanding of current global arms trade dynamics. In the end, Grant draws a fascinating and compelling story of the global arms trade in the nineteenth century and leaves it to the social scientist and policy maker to determine how best to use the lessons of history.

The Aesthetics of International Law. By Ed Morgan. Toronto: University of Toronto Press, 2007. Pp. 272. Price: \$55.00 (Hardcover). Reviewed by Jennie Han.

The Aesthetics of International Law opens with the radical claim that international legal commentary is fundamentally a “fantasized work of art” (p. 5). Ed Morgan, a law professor at the University of Toronto, argues that understanding this art requires that one examine the “form or language of legal doctrine” rather than its “particular politics, economics, or morals” (p. 3). Starting with the thesis that the “aesthetic qualities of the law mirror similar qualities in literature” (p. 29), he identifies in each chapter parallels in rhythm, structure, or imagery that writers of legal commentary and literature employ in their respective works. While Morgan successfully identifies various aesthetic parallels between literary and legal texts, he fails to articulate how these parallels meaningfully inform international law, ultimately failing to show that international law is best read as art.

The topics Morgan addresses are wide-ranging. He pairs issues such as terrorism, war crimes trials, extraterritorial criminal law, extradition and the death penalty, environmental liability, universal jurisdiction, and the law of war with a diverse set of literary figures from Edgar Allen Poe to Henrik Ibsen and Bertolt Brecht, to James Joyce, Mordecai Richler, Vladimir Nabokov, and Thomas Pynchon. The chapters all follow the same template: chapters entitled “Kurt Vonnegut: The Law of War” or “Franz Kafka: Extraterritorial Criminal Law” begin with a paragraph biography of the writer and then proceed to describe a literary element present in both the novelist’s text and a text of international law.

Morgan’s claim is that the aesthetic elements of law and literature mirror one another, and his method of drawing parallels between the law and literature is faithful to this image. However, this methodological faithfulness is counterproductive, if not fatal. It is never clear, for instance, how comparing Ibsen’s invitation to the audience to identify with the “setting, characters, and drama” of the play to Brecht’s theory of theatrical distance and emotional detachment (p. 18) supports or strengthens his conclusions about the Eichmann trial. Morgan describes the trial as “not (only) a vindication of six million lives lost, but a celebration of Jewish sovereignty at last achieved” (p. 26). This conclusion does, admittedly, express a “dualism” of criminal vindication and sovereignty. But it is unlikely that the dichotomy of Ibsenian subjectivism and Brechtian objectivity helps us to identify or understand this dualism, or that the literary duality mirrors the legal one in anything more than a superficial way. In the absence of a sustained argument showing that these dualities stand in an analytically meaningful relationship to one another, Morgan’s claim that an aesthetic reading of international law is the most illuminating one remains unconvincing.

This analytic lacuna exists in nearly all of his chapters. In the chapter on Joseph Conrad’s *Heart of Darkness* and the sources of international law, Morgan draws from the former the lesson, “true meaning is found in the opposite of where it would first appear to lie” (p. 30). In a parallel manner,

international law locates "binding legal authority where the reader was least expecting it, and where as a consequence the notion of systemic norms is least vulnerable to a skeptical attack" (p. 31). This conclusion about the sources of international law is itself quite interesting. But Morgan makes it less so in his attempt to appropriate it as "aesthetic" with a tenuous analytical connection to Conrad's notion of unexpected discovery. Ignoring the theoretical and historical issues implicated in his statement about legal authority in favor of emphasizing the aesthetic connection, Morgan undermines his own insight about the sources of international law.

Morgan's chapter on Edgar Allen Poe and terrorism is especially timely with respect to international law and politics. He argues that in reading Poe's gothic tales, one sees the "hallmark of terror" not as pain and violence, but rather "the transfer of violence from the land of adventure to the reader's own vulnerable and unstable state of mind" (p. 7). Poe's stories are terroristic because "the horror of the tale is turned inward on the reader, who is personally touched by . . . disintegration" (p. 8) (internal quotations omitted). Similarly, terrorism "strikes . . . the law's own stability as law" (p. 9), forcing it into a state of "self-mutilation" as it abandons the "deductive reasoning and linear analysis" that is its source of pride (p. 15). However, Morgan's subsequent analysis fails to support this conclusion about the effect of terrorism on the law. He devotes this short chapter to describing how the narrator in Poe's story, *The Man Who Was Used Up*, and the judges in *Ahmad v. Wigen*, 726 F. Supp. 389 (S.D.N.Y. 1989), are "distracted" in their respective quests for the truth, the former in his search to uncover the mystery of General A.B.C. Smith, and the latter in their attempt to define terrorism. Just as the narrator's interviews turn up conflicting and tangential stories about the General in Poe's story, each of the judges' attempts in *Ahmad* to define terror "contains an allusion to another doctrine in another branch of legal literature" (p. 14). The court's opinion does move between "level[s] of legality," with "international law mov[ing] to the domestic and domestic law mov[ing] to the international" (p. 14). But this "circularity" hardly seems to prove that the law is "overburdened, and therefore exhausted, by the impossibility of meaningful discourse on the subject" (p.14), or that the law reflects Poe's terror. It is equally likely that terrorism, by its nature, lies in some liminal space between the international and domestic and that the court's discussion reflects *this* fact. To conflate international law's project to develop a language to describe terrorism with the law's being "used up" (p. 15) makes an assumption about its historical and innate "stability" that is simply untrue.

Morgan's unwillingness to find aesthetic connections beyond the texts presents yet another problem. Applied only to judges' inability to describe a phenomenon with a stable set of doctrines, the internalized horror and self-mutilation of Poe's gothic terror no longer seem so horrifying. Unproductive as it may be, to call legal circularity terrorism tames the term to the point of meaninglessness. In contrast, Poe's gothic tale may be a revealing lens through which to interpret the political and social *world* that experiences terrorism. But Morgan's strict commitment to drawing parallels from

literature to legal text forecloses the possibility of exploring these connections. Understanding the effects of terrorism on the law and legal institutions may be served quite well by aesthetics, but an aesthetic reading of the world and not just of legal texts.

The Aesthetics of International Law contains some fascinating insights into the law, its history, and its purposes. For example, Morgan hypothesizes that war crimes trials are not simply a means of punishing offenders, but rather a “mode of theatre, seeking a truthful account from the players, but staged in a mode that is carefully circumscribed by convention” (p. 18). This redefinition challenges the perception of legal and political scholars that war trials are useless because they fail adequately to punish perpetrators. Morgan is an impressive interdisciplinary scholar, and his discussions of the theoretical, historical, and psychological elements of legal writing are engaging. In fact, his discussion of literature and law pales in comparison. Morgan’s statement about the indispensability of literature for understanding the law would be much more compelling if he had been less ambitious about establishing aesthetic connections where they likely do not exist. Where the connections are most direct, such as his chapter on the theater of war crimes trials and his section on T.S. Eliot’s modernist response to the historicism of knowledge and meaning and law’s relationship to its own history and context, Morgan’s writing is the most convincing and interesting.

While Morgan’s analysis fails to support his conclusions about international law, these conclusions are novel and interesting enough to validate a message that runs throughout the book: legal scholars should read more nonlegal literature. A familiarity with various literary devices and an understanding of how authors create meaning through language can sensitize scholars to unexpected readings of the law. But then what legal scholars need from Morgan is not a cursory survey of international legal issues placed alongside a survey of literary texts, but a sustained, deep analysis of literature and the aesthetic elements of legal writing so that readers might themselves acquire the openness of vision that comes with the literary and aesthetic perspective.

The Constitution’s Text in Foreign Affairs. By Michael D. Ramsey. Cambridge: Harvard University Press, 2007. Pp. ix, 492. Price: \$65.00 (Hardcover). Reviewed by Becky Perry.

With his provocative book, *The Constitution’s Text in Foreign Affairs*, University of San Diego law professor Michael Ramsey posits a historical answer to the diverse set of constitutional problems in modern foreign policy. Ramsey examines disputes over the role of the President and Congress, the power to enter armed conflict, and the domestic force of international law, in each case deriving insightful solutions from the words, structure, and context of the Constitution.

Despite the immediate relevance to post-9/11 debates about executive war power, few scholars have attempted a comprehensive treatment of foreign

affairs as a work of constitutional theory. Ramsey steps into this gap with a premise designed to challenge conventional wisdom about the coherence of the Constitution. On the first page of the book, he acknowledges that the luminaries of his field—including Louis Henkin and Harold Koh—embrace the prevailing belief that the text of the Constitution is fundamentally incomplete regarding the distribution of power over foreign affairs. In response to such claims, Ramsey argues that “we have too quickly given up on the Constitution’s text” as the source of a basic but balanced structure for foreign affairs power (p. 2). This book, he says, is an attempt to rediscover that constitutional structure.

Conscious of the potentially controversial nature of his argument, Ramsey carefully distinguishes his approach to constitutional interpretation from other constitutional theories. In an online symposium promoting the book’s release, Ramsey explicitly rejected both literalism and originalism, taking pains to distance himself from any perceived political implications of his work. Instead, he embraces “historical textualism,” defined as a focus on the text of the Constitution, with special attention to the way its words and phrases were used before and after ratification.

After sketching his proposed methodology, Ramsey applies this interpretive framework to locate the foreign affairs power within the Constitution. He first rejects the notion of an inherent foreign affairs power, arguing that such an appeal contradicts the plain meaning of the Tenth Amendment, which reserves non-delegated powers to the states. But rather than label the Constitution “fatally indeterminate” in foreign affairs, Ramsey argues that the traditional meaning of “executive power” in Article II can fill the apparent gaps (p. 377). According to Ramsey, the eighteenth-century definition of executive power included power over foreign affairs, as well as the conventional power to execute the law. Thus, Article II, Section 1 (“The executive power shall be vested in a President . . .”) becomes a substantive grant of power rather than a purely formalistic introductory phrase.

Ramsey’s argument is persuasive with regard to the executive powers of diplomacy and communication. His explanation offers a textual basis—in Article II, Section 1—for the diplomatic role of the President, without which the Constitution “would seem to leave a substantial block of power unallocated” (pp. 70-71). His argument about executive war power, however, poses greater textual difficulty because the Constitution transfers significant elements of war power away from the President. The Constitution gives the powers to declare war, approve treaties, raise and support armies, make rules concerning captures, call out the militia, impose trade sanctions, and punish offenses against the law of nations all to the Congress. Ramsey argues that these transfers of executive war power are exceptional and must be strictly construed, while the remaining executive power is reserved to the President through Article II, Section 1.

In this respect, Ramsey appears to neglect the full implications of his textual approach. Minus the Commander-in-Chief Clause and treaty power, the Constitution grants most substantive war-related power to the Congress, not to the President. Given the Article I location of the Necessary and Proper

Clause, Ramsey's premises could well yield the opposite conclusion—that war powers are congressional unless explicitly vested in the President.¹ Thus, while historical textualism provides a satisfying explanation for traditional presidential powers, such as the diplomatic role of the executive, Ramsey struggles to consistently resolve contemporary questions about the constitutional allocation of war power.

Despite these lingering issues of application, Ramsey's method yields results that defy the boundaries of traditional political parties. Some of his doctrinal arguments would undoubtedly alienate political liberals. Under Ramsey's account of the Constitution, for example, the President would have power to fight undeclared defensive wars and to terminate treaties without congressional approval, while the powers of the states would not be constrained by international law.² But other outcomes of Ramsey's historical textualist approach would equally estrange political conservatives. The President's executive power in foreign affairs would have to be tied to a particular treaty or statute—precluding most unilateral executive action—while the courts would maintain a substantive role in foreign affairs law.³ Thus, Ramsey maintains his claim to political neutrality by successfully eluding partisan stereotypes.

In the structural picture that emerges, the Constitution grants important foreign affairs powers to the executive, but reserves a ballast of independent powers for the Congress. To Ramsey, this prudent separation is the consequence of a founding generation deeply concerned about the state of post-colonial foreign affairs. Given the international problems that plagued the impotent Articles of Confederation, Ramsey argues, "it seems most unlikely that the founding-era Americans simply forgot to provide a foreign affairs framework for their new government" (p. 2). More likely, he posits, the founders chose to employ the same strategy of checks and balances that characterizes the rest of the Constitution, thereby creating multiple and independent centers of foreign affairs power.

To critics, Ramsey's case is an oversell, premised on a historical record rife with ambiguity. Does the existence of historical disagreement about the definition of executive power preclude the type of certainty Ramsey's analysis requires? Is constitutional history simply too messy to permit accurate judgments about original meaning? To deflect such skeptical questions, Ramsey inserts a brief but important caveat into his argument, disavowing any normative implications that might be derived from the book. According to Ramsey, his is a purely descriptive project, designed to identify—not to mandate—the historical foreign affairs structure of the Constitution: "I do not mean to enter into the complex debates over the proper grounding of modern constitutional law," he says (p. 2). While the disclaimer cleverly shields him from criticism, Ramsey cannot (and should not) so blithely opt out of the modern constitutional debate. His careful and even-handed scholarship

1. Andrew Kent, *The President's Residual Power to Use Military Force*, OPINIO JURIS, Aug. 1, 2007, <http://www.opiniojuris.org/posts/1185968671.shtml>.

2. Martin Flaherty, *Can History Determine Textual Meaning?*, OPINIO JURIS, Aug. 1, 2007, <http://www.opiniojuris.org/posts/1185988281.shtml>.

3. *Id.*

demonstrates that he is fully capable of addressing not only the descriptive content of the Constitution, but also the normative implications of that content for current constitutional questions.

In the end, the persuasive value of *The Constitution's Text in Foreign Affairs* turns on the truth of Ramsey's core claim about the coherence of the Constitution. To some historians, such a commitment embodies the "aesthetic fallacy"⁴ of infatuation with the elegance and clarity of a merely political document. To Ramsey, coherence is the logical product of the intellect of the framers and the weight of the constitutional moment.

Extraordinary Justice: Military Tribunals in Historical and International Context. By Peter Judson Richards. New York: New York University Press, 2007. Pp. 267. Price \$45.00 (Hardcover). Reviewed by Danny Habib.

That Peter Judson Richards has great confidence in the efficacy and fairness of military tribunals and a corresponding impatience with civil libertarian critics emerges only gradually and subtly from his book, *Extraordinary Justice: Military Tribunals in Historical and International Context*. Not until quite near the conclusion does Richards let slip the full measure of his exasperation. In explaining Alexander Hamilton's support for a death sentence handed down by a Revolutionary War military court to an army major found guilty of treason, Richards observes that Hamilton, an artillery captain as well as a trial lawyer, "was not assessing the facts at an abstract distance, from the peaceful groves of an academy rendered safe by other men's blood, but as an officer charged with defending and protecting lives in a perilous war" (pp. 177-78).

Blood and peril, the exigencies of war, states of emergency, catastrophic threats: all these permeate Richards's survey of military tribunals over the past two centuries, from the Mexican-American War to the War on Terror. Richards finds legal justification for "these courts-in-extremity" (p. 3) primarily in the crisis conditions that prompt their creation and believes that these extraordinary mechanisms remain legitimate only to the extent that war has closed the civilian courts, caused them to malfunction, or requires functions (processing combatant defendants quickly, protecting classified information, judging violations of the laws of war) beyond their competence. At the end of this historical trajectory, Richard considers the Combatant Status Review Tribunals at Guantanamo Bay, Cuba, deployed now as ever, in response to a national security crisis. In his view, only the misplaced concerns of libertarians and the improper intervention of the federal judiciary have retarded the tribunals' efforts: while "America and its Western allies wrangle over the sufficiency of procedural and substantive safeguards for the trial and treatment of enemy detainees . . . illegally organized non-state underground actors take captive American and allied troops . . . and then execute them without trial" (p. 163). This is a seductive argument and it describes a danger

4. *Id.*

that infects all extrajudicial tribunals, none more so than Guantanamo's: when many and indeterminate dangers loom, and when the alternative is the deliberate machinery of the civilian system with its panoply of procedural safeguards for unsympathetic defendants bent on doing lethal harm, tribunals seem an attractive alternative.

Extraordinary Justice is arranged as "a series of discrete, modern examples—from the American Civil War, the Second Boer War, the First World War and the Second World War" (p. 7), and the tendency of tribunals to expand beyond their original jurisdictional boundaries is evident from the start. In the Civil War, military commissions were administered under martial law, principally in the anarchic border territory of Missouri. They arose in response to guerrilla-type violence (bridge burning, destroying train tracks) wrought by non-uniformed Confederate partisans and were "a means of providing a modicum of stability in the midst of appalling brutality and civil disorder" (p. 28). Richards sees mitigation for their summary character in the mandatory, if minimal, procedures that accompanied them: they kept written records, superior military and civilian officials used those records as the basis for review and discretionary grants of clemency, and capital sentences required presidential approval. Yet the commissions were soon used elsewhere and persisted into Reconstruction, when southern states under martial law saw their civilian courts, often unwilling to apply race-blind justice, supplanted by the Army's. Enthusiastic about the latter application "as an important instrument . . . for regulating volatile racial tensions in areas under the army's jurisdiction" and "a significant interim measure for obtaining decently fair, impartial conditions for administering justice at a time when such commodities were in short supply" (p. 43), Richards quiets concerns about the extended duration of military process by broadening the conceptual sweep of crisis conditions.

Richards distinguishes between peacetime civilian justice, which has inherent value as a guarantor of parties' rights, and purely instrumental wartime adjudication, a tool of the military commander. The distinction originates in Abraham Lincoln's familiar claim that liberty presupposes national security. Richards paraphrases Lincoln's Corning Letter: "Once the dangers of bloody internecine war had been safely negotiated, the blessings of civil liberties could be duly chartered, but to absolutize the nature of the protections was to ignore the very real preconditions of military and political security that alone could give such liberties a chance of survival" (p. 34). Absolute assurance of due process simply does not comport with military necessity, Richards argues, and wartime tribunals should be measured not against an abstract standard, but in the context of the strategic choices faced by battlefield commanders: "The yardstick of procedural correctness simply fails to provide a sufficient index of the worth and value of a given proceeding, as an historical phenomenon taking place within, and itself shaping, the unfolding international political and military matrix" (p. 175). Hence, the permissiveness with which Richards regards tribunals' procedural defects: the right to a jury trial pales in comparison to the need for rapid disposition of cases and the conservation of military resources, the right to

confront inculpatory evidence falls when set against the state's interest in secrecy. To provide a concrete illustration, Richards points to the criminal trial of the Kenya and Tanzania embassy bombers, during which the United States put the al Qaeda training manual into evidence, which may have allowed al Qaeda to change its operational practices in response.

It is precisely this decoupling of military tribunals from an anterior value system and their evaluation on a flatly instrumental basis that allows their expansion beyond the battlefield. Richards adduces efficiency justifications for the endurance of the French *conseils de guerre* of World War I well into the armistice period in the occupied German Rhineland, and for the war crimes prosecutions after World War II. The immense volume of cases on the docket dictated streamlined, speedy procedures, and military personnel in the field "were often the only practicable agencies for the investigation, discovery and, in many cases, adjudication of war crimes" (p. 113). A chapter on the Boer War—of particular contemporary pertinence because it devolved into a protracted counterinsurgency struggle with frontlines at once everywhere and nowhere—discloses the slipperiness of such an approach. Faced with the prospect of resistance in the Cape Colony, relatively peaceful but considered vulnerable, the British Army created military courts beset with "occasional injustice, administrative deficiencies and arbitrariness in the process" (p. 55). Because "a new reality of armed conflict [was] its expanding, shifting spatial and temporal boundaries" (p. 51), amplified powers were wielded over a wider area. "[T]here had to be an element of positive, preemptive power attached to . . . emergency response. This required greater elasticity in assessing the geographical limits of the area circumscribed by necessity" (p. 69). The book's broadest claim here emerges: that military authorities (or the civilian commander-in-chief) should be left significant latitude to determine the theater of war, to prescribe preemptive measures necessary to protect national security, and to construe the state of emergency as lasting perhaps indefinitely.

The specter of a sprawling, endless state of emergency haunts the rough military justice now administered at Guantanamo. Richards sees asymmetric conflict with an irregular enemy that does not consider itself bound by the laws of war and wonders at the solicitude shown for the detainees by federal judges: "[T]he characteristic judicial focus on private, individual claims of right ill suits members of the judiciary to levels of oversight that have direct bearing on the tactical and strategic dimensions of military decision-making in war" (p. 160).

But the Combatant Status Review Tribunals' significant deviations from the civilian standard—the admissibility of hearsay and coerced evidence, the government's power to withhold classified evidence, the presumption in favor of the accuracy of the government's case—result in a meager approximation of justice. Richards's reply to this argument is that the Constitution does not protect unlawful enemy combatants, and moreover, that the United States does not owe them anything like a trial process, since they, our enemies, do not reciprocate. A belief that procedure guards the rights of parties in jeopardy rejects a reciprocity test, however; the United States designs adjudicatory

processes based on its own values, not those of a terrorist enemy. But even accepting Richards's instrumental evaluation of military tribunals, he misses at least one crucial variable: at Guantanamo, it is often the detainees' very classification as unlawful combatants that is itself at issue, a difficulty that previous military commissions dealing with uniformed, regular soldiers have not confronted.

Richards's ultimate ambition, which could be described as, "to wrest the supervision of the conduct of war away from judges, and put it back with the political branches," (p. 160) threatens to mothball civilian justice in potentially limitless ways. Richards makes no attempt to provide for a sunset to the tribunal system, nor restrain the executive's power to define it. War of open-ended duration, with battlefields as plausibly found in American airports as in Afghanistan, may permit military commissions to become normative. Put another way, how long before extraordinary justice becomes merely ordinary?

Legacy of Ashes: The History of the CIA. By Tim Weiner. New York: Doubleday, 2007. Pp. 671. Price: \$27.95 (Hardcover). Reviewed by Elbridge A. Colby.*

Apparently it is still considered news that the CIA is neither omniscient nor omnipotent. Tim Weiner, a veteran reporter on intelligence for *The New York Times*, gamely states this point in the introduction of his new book, *Legacy of Ashes: The History of the CIA*, arguing that the United States "has failed to create a first-rate spy service" and that this "failure constitutes a danger to the national security of the United States" (p. xiii). Five hundred pages later, after cataloguing the ups and many downs of the world's most famous (or infamous) secret service, Weiner concludes that the U.S. intelligence community, and thus "we Americans[,] still do not understand the people and the political forces we seek to contain and control" (p. 514). This is a grave peril, he argues, because "the war in which we are now engaged may last as long as the Cold War, and we will win or lose by virtue of our intelligence" (p. 514).

Weiner has been garnering a lot of accolades for his book, perhaps because it echoes the conventional wisdom—that U.S. intelligence has "failed." This is a convenient and "punchy" explanation for the depressing performance of American intelligence agencies over the last sixty years. But it is also grossly oversimplified and dangerously misleading. American intelligence, on the whole, has not "failed" so often because of incompetence, a hard-drinking workforce, or a cavalier attitude, but because of the inherent difficulty of its task: predicting the future. And predicting the future, especially when the relevant information is being deliberately hidden, is

* The author worked from 2003 to 2006 in the U.S. intelligence community, including at the Central Intelligence Agency and the Office of the Director of National Intelligence.

notoriously hard. Uncertainty, irreducible complexity, and surprise are built into the rules of the game.¹

Weiner's book is written in a constant tone of appalled contempt for the CIA's ineptitude, but he might have reached a more realistic picture of the constraints facing the Agency—and therefore appropriate standards by which to judge it—if he had applied some basic tools of logical and evidentiary reasoning. First, Weiner might have applied a good rule for analyzing organizational performance: if large numbers of reasonably competent people are failing, the problem is probably systematic. Second, he never benchmarked the Agency's performance against its peers. If he had done so, he would have seen that the intelligence services of Britain, France, Germany, and even the vaunted Soviet Union, were all also "failures" when measured against his exacting standards. To name just a few: no intelligence service correctly understood or predicted Saddam Hussein's policies on weapons of mass destruction; the legendary Israeli intelligence system was fooled in the Yom Kippur War of 1973; the Japanese gravely misunderstood the American reaction to their attack in 1941. No intelligence agency has managed to tame the untamable problem of "anticipating surprise."

Further, Weiner doesn't factor in the difficulties presented by the CIA's targets: the world's most closed, totalitarian regimes. He chastises the Agency for not knowing more about the Soviet Union throughout the Cold War and gives it little credit for signal triumphs such as obtaining Khurshev's 1956 speech disavowing Stalin. He makes little allowance for the fact that the Soviet Union was a police state organized to prevent the release of information that had readily murdered *millions* of its citizens on the (fabricated) grounds that they were *spies*. The Soviets created whole secret *cities* for their strategic and space programs. Weiner at one point quotes an Agency Soviet expert stating that the CIA would have needed to "recruit a member of the Politburo" to get what was required. Needless to say, the chances of recruiting a member of the Soviet inner sanctum of power were low. Nor is the Soviet Union unique: the key intelligence targets of the United States tend to be closed, totalitarian societies paranoid about security, and a fair judge of the Agency's performance needs to take this reality into account.

Even when Weiner concedes that the Agency was successful, he tends to cast it as a malevolent actor. The Agency, it seems, can do no right. In discussing the CIA's successful covert political actions in Italy, France, Germany, Japan, Iran, Guatemala, the Philippines, and elsewhere, he takes the Agency to task for undermining national self-determination and being complicit with authoritarian governments. But if the Agency's purpose was to combat Soviet communist influence globally, and if, as the Doolittle Report of 1954 (a report that Weiner cites approvingly) recommended, the United States would have to adopt methods previously forsworn, then the grim means used in these countries should weigh in the Agency's favor, at least when calculating effectiveness. It is hardly fair to take the Agency to the woodhouse

1. For a fuller explication of the author's views on this subject, see Elbridge A. Colby, "Making Intelligence Smart," 144 POL'Y REV. 71 (2007), available at <http://www.hoover.org/publications/policyreview/8817787.html>.

for ineffectiveness and then to discount successes because they were won using unpalatable means.

This tension between calling for both effectiveness and for purity illuminates a major contradiction in much current criticism of the U.S. intelligence community, which calls for *both* a more effective *and* a purer intelligence community. The disappointing catalog that Weiner presents should teach us that such a convenient mix is not an option.² The Agency has never been all that effective when measured against Weiner's implausibly exacting standards, and, when it has been effective, success has often stemmed from ways and means its critics find distasteful.

So what inferences should be drawn from the CIA's historical record? Weiner seems to think that a redoubling of efforts by better, smarter, or more culturally-attuned people at the CIA will do the trick. But is there any real reason to think that previous generations of CIA officers were especially incompetent or malevolent? The sample's size, variability, and length should rebut that notion. A more appropriate inference would be that the problematic variable is not the quality of the troops, but rather some combination of the inherent limitations of intelligence and the methods employed. And while the first of these obstacles is insurmountable, it seems unlikely that Weiner wants to go down the path of the second, which would seem to call for the very methods that he deplores. If he is unwilling to embrace these tactics, he should be far more charitable to the CIA, which must operate within severe constraints.

In foreign policy, knowing a little is better than knowing nothing. Still, the deeper inference to draw is that intelligence is starkly limited in what it can promise and that the chances of surprise can be lessened but never blotted out entirely. This would suggest that we not rely too much upon intelligence—important as its role may be—to ensure our security. Despite Weiner's claim that the war on terror will be won or lost based on the performance of our intelligence services, the U.S. government knows that intelligence is not as central to our national security as some seem to think, which is why the low quality of American intelligence so often bemoaned by U.S. presidents has rarely, if ever, affected our basic security. The reason for this is that America's safety rests on firmer grounds—especially the ground of the credible threat of force. But making this threat of force credible, effective, and successful is a difficult task and requires our making tough and sometimes unpalatable tradeoffs. Intelligence will play only a supporting, if indispensable, role in this effort. Thus, Weiner's book, despite its mistaken gloss, should make clear that vesting our hopes for security primarily in intelligence would be foolish. But it should also awaken us to the tough but necessary set of choices that we will need to make as the threat of catastrophic destruction becomes more and more stark.

2. For a fuller discussion of the necessary tradeoffs between effectiveness and what is here termed "purity," see RICHARD A. POSNER, *COUNTERING TERRORISM: BLURRED FOCUS, HALTING STEPS* (2007) and RICHARD A. POSNER, *NOT A SUICIDE PACT: THE CONSTITUTION IN A TIME OF NATIONAL EMERGENCY* (2006).

